THURSDAY, JANUARY 12, 1905.

SCIENTIFIC THOUGHT IN EUROPE.

A History of European Thought in the Nineteenth Century. By John Theodore Merz. Vol. i., pp. xiv + 458; vol. ii., pp. xiv + 807. (Edinburgh and London: William Blackwood and Sons, 1903–4.)

A NEWSPAPER review of this book has come into our possession, which gives the impression that its most prominent feature is the treatment of biological questions such as the Darwinian theory. Doubtless the reviewer was a biologist. His remark that "the book is not a very easy one to read" is, however, very true.

Now to the present writer the feature which appears most noteworthy is the author's intimate knowledge of *mathematics,* as revealed in his masterly expositions of the development of all branches of mathematical thought during the last century. Probably an exhaustive account of this work could only be given by a number of different reviews written by specialists in different subjects, and such reviews would be so different that it would be difficult to realise that they all referred to the same book. The course we propose to follow is to give a general outline of the scope and subject-matter of the book, to scrutinise a little more closely the portions devoted to mathematics and mathematical physics, and to subject such branches as thermodynamics and kinetic theory to a still closer scrutiny.

At the outset (pp. 24–27), Dr. Merz is confronted with the difficulty that he can find no precise equivalent in French or German for our English word *thought*; for instance, he says:—

> "No other language has a word so comprehensive, denoting at once the process and the result, the parts and the ideal whole of what is felt and meant. . . ."
>
> "And yet I think I am right in saying that the conception of thought in the sense in which I am using it is truly an outcome of international, not of specifically English progress, and belongs mainly to the period of which I am treating. . . ."

What thought precisely is the author considers impossible to define, but it is only thought which renders the phenomena of nature intelligible, as he says (p. 2):—

> "That which has made facts and events capable of being chronicled and reviewed, that which underlies and connects them, that which must be reproduced by the historian who unfolds them to us is the hidden element of thought."

It is the object of these volumes, as the author remarks on page 13,

> ". . . . . to rescue from oblivion that which appears to me to be our secret property; in the last and dying hour of a remarkable age to throw the light upon the fading outlines of its mental life; to try to trace them, and with the aid of all possible information gained from the written testimonies or the records of others to work them into a coherent picture, which may give those who follow some idea of the peculiar manner in which our age looked upon the world and life, how it intellectualised and spiritualised them."

On p. 34 he says:—

> "A history of this thought will be a definition of thought itself."

In order to limit the scope of the inquiry, Dr. Merz confines his attention to European thought, and of this, again, he only selects the central portion, the thought embodied in French, German, and English literature. Accordingly the first three chapters deal with the scentific spirit in France, Germany, and England respectively. This order of arrangement is a fitting one, and well brings out all that has been said by various writers about "England's neglect of science." Thus (p. 75):—

> "Compared with Germany in philosophy and with France in science, England during the early part of the century appears remarkably unproductive. English science and English philosophy had flourished in the seventeenth and eighteenth centuries and leavened the whole of European thought, but in the beginning of our period we find neither represented by any great schools. The great discoveries in science belonged to individual names who frequently stood isolated; the organisation and protection which science could boast of in France was then unknown in England; into popular thought it had hardly entered as an element at all."

It is to France that we must turn in order to find what might be described as a *national* scientific spirit, and this spirit was very largely the outcome of the foundation of the Paris Academy of Sciences.

> "Whilst the Royal Society of London only received a charter, and existed by the entrance payments and contributions of its own members, augmented by private donations; the Paris Academy had as far back as 1671 received the funds with which to commence its labours in connection with the survey of the kingdom and its extensive dependencies." . . . "It was almost exclusively by these observations that the data were found with which to substantiate Newton's mathematical reasoning; in his own country that fruitful cooperation which can only be secured by an academic organisation and the endowment of research was wanting" (p. 99). "In two important departments—the popularisation and the teaching of science—France for a long period led the way. A general interest was thus created in the proceedings and debates of the Academy. . . . ."

In the present connection are cited Laplace's "Mécanique Celeste," and the development of the analytical methods rendered possible by Leibnitz's invention of the calculus, about which we are told (p. 101),

> "No learned body did more than the Paris Academicians to perfect (with purely scientific interest) this new calculus, which in the course of the eighteenth century had in the hands of Lagrange been adapted to all the purposes and problems contained or suggested in Newton's Principia."

As another illustration we take the popular interest which centred round Laplace's discovery of the calculus of probabilities (pp. 120 *et seq*).

Passing on to Germany we find national interest converging towards another equally important centre, namely, the university system, which is unique of its kind. This system was perfected in the eighteenth, and fully developed at the beginning of the present century. It is essentially a training school of research and its ideal is expressed in the word *Wissenschaft.* This word, Dr. Merz considers, "cannot be defined by any single word of the English language."

> "In fact, the German word for science has a much wider meaning than *science* has in French or English, it applies alike to all the studies which are cultivated under the roof of an 'alma mater'; it is an idea specially evolved out of the German university system, where theology, jurisprudence, medicine and the special philosophical studies are all held to be treated 'scientifically' and to form together the universal, all-embracing edifice of human knowledge" (p. 170).

It was not, however, until the second quarter of the century that the scientific spirit had entered the universities.

> "During these twenty-five years Gauss lived and soared in solitary height—a name only to the German student as Euler had been before him." "The man to whom Germany owes its first great school of mathematics was Jacobi" (pp. 184–5).

> "German science was essentially cosmopolitan, and the absence of a central body like the Paris Academy, led to an important result, the publication of a large number of periodicals devoted to special branches of science."

Turning to Great Britain the author says (p. 225):—

> "Considering that the great scientific institutions of the Continent—the Paris Institute, the scientific and medical schools in Paris and the German universities—have done so much for the furtherance of science and the diffusion of the scientific spirit, it is natural that we should ask, What have similar institutions done in this country?"

A perusal of this chapter leads to the general conclusion that a "national" scientific spirit has never existed in our country. The records of the great discoveries made in Britain during the half-century ending 1825 (given in a footnote on p. 229) show that in that period hardly a year passed without some great scientific discovery being made by an Englishman, and fully justify the statement that

> "England had during the early part of the century in all but the purely mathematical sciences a greater array of scientific names of the first order than Germany, and nearly as great an army as France."

And yet we find the works of these writers quite unknown in their own country, and in many cases only rescued from oblivion by falling into the hands of the Continental schools of science. We have only to instance Dr. Merz's references to the difficulties encountered by Young, Green, Babbage, Boole, Dalton, Faraday, and a host of others, and then to refer to foreign opinions on English science, as expressed by Cuvier and Prof. Moll, and quoted (pp. 235–7), as evidence of the high estimation in which British scientific work was held on the Continent. The lack of stimulus to scientific research, the absence of higher mathematical studies, were peculiarly noticeable in the two older universities, where traces of the same spirit survive to this day in spite of the internationalising influences which have played such an important part in recent scientific work. If Britain played a prominent part in the origination of the metric system, and if Continental nations base their zone system of time on the meridian of Greenwich, no better evidence of the general *national* apathy to science could be adduced than the fact that Britain is one of the few European States which have not yet universally adopted either of these systems.

The last two chapters of vol. i. are devoted to "The Astronomical View of Nature" and "The Atomic View of Nature," while chapters vi. to xii. in vol. ii. deal with the "kinetic or mechanical," the "physical," the "morphological," the "genetic," the "vitalistic," the "psycho-physical," and the "statistical" views of nature. These chapters refer more especially to the second half of the present century, and it is in them that we feel ourselves compelled to single out a few selected points rather than attempt to cover the whole range of subject-matter.

It is well known that many of our leading scientific ideas can be traced back to very ancient sources; as instances, Dr. Merz refers to the law of gravitation and the atomic theory as known to the Greeks and Romans, the kinetic theory as suggested by Heraclitus, the vortex atom theory as forestalled by Descartes and Malebranche (pp. 312–4). In passing judgment on these prior claims, Dr. Merz very rightly remarks:—

> "It is the scientific method, the exact statement, which was wanting, and which raises the vague guesses of the philosophical or the dreams of the poetic mind to the rank of definite canons of thought, capable of precise expression, of mathematical analysis and of exact verification." "In every case the awakening touch has been the mathematical spirit, the attempt to count, to measure, or to calculate."

Those who flood our breakfast tables with "new" theories of the ether or designs of flying machines only constructed on paper will do well to bear these remarks in mind.

Let us now examine how Dr. Merz treats the second law and the ideas of temperature and entropy. In commenting on the work of Lord Kelvin and Clausius, he says (p. 128):—

> "The result was the doctrine of the 'conservation of energy'—not of heat as Carnot had it—and the embodiment of the two correct ideas contained independently in Carnot's and Joule's work in the two well-known laws of thermodynamics—viz. the conservation, equivalence and convertibility of energy as expressed in the first law and the doctrine of the availability of energy as expressed in the second law."

In speaking of entropy (p. 169) he is no less definite in associating that conception with unavailable energy, and he only falls into a pitfall on p. 594, where he speaks of "entropy (or energy which is hidden away)" as if the two were identical and did not differ by a temperature-factor. But the footnote on p. 189 of Maxwell's "Heat," seventh edition, shows that in this he has erred in good company. In the footnote on p. 315, in discussing the absolute scale of temperature, he is more unfortunate. The scale "in which every one degree had the same dynamical value" was not the *present* absolute scale (which approximates fairly closely to the gas scale), but Lord Kelvin's *first* absolute scale, published in 1848, in which the absolute zero is not $-273°$, but minus infinity.

Of the application of statistical methods to the kinetic theory we can speak equally well in regard to the completeness with which the author has traversed the literature of the subject. We do not find any reference to the underlying *assumption* which has up to the present been unearthed in every attempt to treat the problem mathematically. But this is hardly a point on which anyone but a specialist could be expected to light, and the majority of specialists make the assumption without knowing it (*pace* Burbury's criticisms).

The last chapter but one deals with the development of mathematical thought. We have selected for special examination the portions dealing with Cantor's researches on the transfinite and the continuum, and we find the subject treated in such a way as to present a clear and definite picture to one who has not specialised in this difficult branch of mathematical thought. The last chapter contains a retrospect and prospect.

We must not omit to mention what is, perhaps, as important a feature as any, namely, the footnotes, which occupy a considerable proportion of the whole book, and constitute a kind of historic encyclopædia.

We do not believe in filling reviews with lists of misprints, but the "Racket" (index, p. 800) may perhaps better describe Stephenson's locomotive than its correctly spelt name. A more serious defect is that these two large and bulky volumes have been issued with the pages uncut, and readers have to waste much time in doing what is the proper work of the guillotine before they can begin the book. This want of thought on the part of the publisher (on his own head be it—*i.e.*, the guillotine) constitutes a serious obstacle to the attempts made by scientific workers of the present day in endeavouring to cope with the ever-increasing mass of literature that accumulates before them.

G. H. BRYAN.

THE PROBLEMS OF VARIATION.

Variation in Animals and Plants. By H. M. Vernon, M.D. The International Scientific Series. Pp. ix+415. (London: Kegan Paul and Co., Ltd., 1903.) Price 5*s*.

THIS little book meets a real want. The frequent discussions of recent years upon the problems of evolution have been followed with much interest by an increasing number of readers and listeners, with the desire but often the inability to understand. A very large amount of interest and stimulus has been excited by such questions as acquired characters and their transmission or non-transmission by heredity, the continuity of the germ-plasm, physiological selection, continuous or discontinuous evolution, De Vries's experiments and views on mutation, the Mendelian hypothesis as opposed to that of Galton and the bearing of the great array of facts, the fruits of observation and experiment conducted by those who take opposite sides in the controversy. The present writer has often been surprised at the keenness of the interest which can coexist with an almost complete lack of knowledge of the essential details, and he feels that the present work provides precisely the information that is required—a clear, accurate, and dispassionate statement, not too long or too detailed, of researches and reasoning upon problems connected with variation.

The notable success of Section D during the late meeting of the British Association at Cambridge provides an excellent illustration of the wide and deep interest excited, at the present moment, by the last of the subjects mentioned above, and was in itself in some measure an answer to the complaint in the presidential address that insufficient attention was paid to the re-discovered discoveries of Mendel. The subject was new to probably a large proportion of the audience: those among them who had taken the opportunity of reading the fourth and fifth chapters (on blastogenic variation) of this work must have felt that they were thoroughly prepared to follow the discussion in all its detail.

The book is divided into three parts, of which the first, dealing with the *facts of variation*, contains three chapters, on the measurement of variation, dimorphism and discontinuous variation, and correlated variation respectively; the second, the *causes of variation*, includes two chapters on blastogenic variations, one on certain laws of variation, and four respectively treating of the effects of temperature and light, moisture and salinity, food and products of metabolism, and conditions of life in general; the third, *variation in its relation to evolution*, is considered in chapters on the action of natural selection on variation, and on adaptive variations.

The author wisely uses the word "hybridisation" very prominently in his account of Mendel's researches and conclusions. In the comparison between the Galtonian and Mendelian views of heredity an important difference is sometimes lost sight of—the present writer does not remember hearing it expressly mentioned, although it was certainly implied, at Cambridge. The former view is, at any rate chiefly, built upon the results of interbreeding between individuals separated by ordinary differences, the latter upon interbreeding between individuals separated by differences comparatively large. "Ordinary" differences are the points of distinction—generally small, mainly differences of degree—by which we discriminate between the individuals of a species forming a single compact mass, or if the species be broken up into two or more masses—then between the individuals within each of them. The larger differences alluded to are the points of distinction—generally large, frequently differences of kind—between the individuals of one mass ("species," "race," or "breed") and those of another, or between the ordinary individuals of a mass and those sudden large departures from its type which are apt to appear spontaneously in its midst. Even when breeds or races are distinguished by a test apparently so superficial and unimportant as colour, we are probably often confronted by the mere outward sign of inward and important distinction.

If the Mendelian view should hereafter be established beyond the possibility of doubt, there will still remain the interesting question of the part it has played in evolution. This is very largely the attempt to decide whether Darwin's earlier or later views were correct, whether evolution proceeds from the selection of large variations, "as when man selects," or from the selection of ordinary individual differences as